CANCER RISK FROM EXPOSURE TO OCCUPATIONAL ACRYLAMIDE

Recently the results of a comprehensive epidemiological follow up study of cancer mortality in cohorts with occupational exposure to acrylamide was published. With the exception of a weak significance for a raised incidence of pancreatic cancer the study arrived at and largely at the conclusion that there is “little evidence for a causal relation between exposure to acrylamide and mortality from any cancer sites”. The study updates and confirms an investigation 10 years earlier of the same companies. The analysis was based on standardised mortality ratios (SMRs) in comparison with United States national or relevant county mortality statistics. It exemplifies the shortcomings of epidemiological study design and methodology, to this kind to demonstrate that moderate influences of specific causative factors on cancer mortality or incidence. The investigators state that they have carried out the most definitive study of the human carcinogenic potential of exposure to acrylamide conducted to date. The results, however, pose questions. Could unacceptable risks be detected? Which risks would have been expected? For the workers in the United States the average cumulative exposure is given as 0.25 mg/m³·y. (We assume this to correspond to exposure of the whole factory staff to 0.25 mg/m³ for 365 hours working days). At an alveolar respiration rate of 0.2 l/kg/min this exposure would mean a cumulative uptake of about 9 mg acrylamide per kg body weight. This dose corresponds to a lifetime (70 years) uptake of 0.35 µg/kg.d. According to the estimation of the United States Environmental Protection Agency this would correspond to a cancer risk of 1.6·10⁻⁵. An estimate based on the multiplicative model would arrive at roughly a 3 times higher risk, 5·10⁻⁵. With a cancer mortality in the western world countries of 0.18, these figures correspond to a 1%–3% increase of the cancer mortality risk (RR)—that is, an RR of 1.01–1.03. As about 1.1–1.2, it is a pertinent question whether the related findings are interpreted in the light of their hypothesis generating nature.

Correspondence to: Dr M Törnqvist
Stockholm University, S-106 91, Stockholm, Sweden


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Marsh et al reply

Granath et al take issue with our update of a cohort of acrylamide workers from three United States plants’ claiming that “it exemplifies the shortcomings of studies of this type to detect moderate influences of specific causative factors on cancer mortality or incidence.” To support their case they overlooked a small but “unacceptable” increase in cancer risk, they performed a crude quantitative risk assessment. Granath et al suggested that we perform within cohort dose-response analysis with all malignant neoplasms as the end point as a means of attaining greater statistical power. They further contend that initial focus on specific cancer sites implicated in previous experimental animal studies is mostly a consequence of the pattern of background incidences in the animal strain used. Although choosing a generic health outcome such as all cancer sites combined will certainly increase statistical power, it also greatly reduces the ability to evaluate the all important specificity of an exposure-response relation. It is unlikely that even the most potent carcinogenic agent will increase the risks of all cancer sites to a level that can be detected with epidemiological methods.

We were fully justified in using cancer site specific findings as the focus of our epidemiological investigation. The use of cancer site specific findings from experimental animal studies to formulate initial testable aetiologocal hypotheses for human studies is an effective, accepted method commonly used in occupational epidemiological research. Animal studies can be particularly helpful when investigators are faced with a paucity of existing epidemiological knowledge concerning the human risks of acrylamide. This practice does not preclude, however, the exploratory investigation of other non-implicated sites as long as the related findings are interpreted in the light of their hypothesis generating nature.

We agree that for many of the initial cancer sites examined in our study, the statistical power to detect a moderate excess in mortality (1.5 to twofold or greater) was low, a point considered in the discussion section of our paper. However, the purpose was to detect a twofold or greater excess in lung cancer, the end point of primary concern, at the one sided 5% significance level was in the excellent range (0.87), as would be the power to detect a similar excess of pancreatic cancer in a future update of this cohort.

Granath et al overlook a fundamental point—occupational cohort studies of the type we used to evaluate cancer mortality risks among workers exposed to acrylamide are neither designed nor necessarily well suited for quantitative risk assessment. Occupational cohort studies are purposely not designed to detect small excesses in the range of 5%–15% deemed by Granath et al as unacceptable. The primary reason for this is that excesses of this magnitude could easily be due, at least in part, to one or more confounding factors. Observational epidemiological studies usually cannot discriminate among such small mixed effects, and are generally most useful for detecting increases in risk that exceed 50%–100% as these are unlikely to be due to uncontrolled confounding. Considerations of statistical power notwithstanding, the fact remains that our study is the largest and most comprehensive study of exposure to acrylamide conducted to date,

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Dose-response relationship between acrylamide and pancreatic cancer

In their 1999 study of workers exposed to acrylamide, Marsh \textit{et al} conducted an SMR analysis, and fitted several relative risk regression models to the data. In each analysis, they found the risk of pancreatic cancer increased by about twofold for workers in the highest cumulative exposure group, but risk of pancreatic cancer did not increase monotonically with cumulative exposure in any of their analyses. Duration of exposure was monotonically related and mean intensity showed a nearly monotonic relationship with risk of pancreatic cancer.

The cut-off points Marsh \textit{et al} chose for the exposure-response pattern are based on multiples of current and proposed regulated levels of exposure intensity.\textsuperscript{1} Because these cut-off points resulted in small numbers of expected deaths in the low and intermediate exposure groups, 1.08 and 2.74 respectively, we have regrouped the data to attempt to obtain more stable standardised mortality ratios (SMRs). These results are presented in table 1 and indicate a monotonic dose-response pattern with the SMRs increasing from 0.80 to 1.31 to 2.26.

\textbf{Table 1} Observed deaths, expected deaths, and SMRs for cancer of the pancreas, all United States workers, 1950–94, local county comparisons, two lowest exposure groups combined

\begin{table}[h]
\begin{tabular}{|c|c|c|c|}
\hline
Cumulative exposure (mg/m\textsuperscript{3}·y) & Obs & Exp & SMR 95\% CI \\
\hline
<0.001 & 30 & 37.50 & 0.80 to 1.14 \\
0.001–0.29 & 5 & 3.82 & 1.31 to 3.05 \\
>0.30 & 9 & 3.98 & 2.26 to 4.29 \\
\hline
\end{tabular}
\end{table}

In part based on the absence of a pattern of monotonically increasing risk with increased cumulative exposure, Marsh \textit{et al} argue that “our findings for cancer of the pancreas should be interpreted with caution, in the context of an exploratory analysis to generate hypotheses.”\textsuperscript{6} Nevertheless, given the sufficient evidence in experimental animals for the carcinogenicity of acrylamide,\textsuperscript{7} this study plays an important part in the evaluation of safety for occupational exposures to acrylamide.

When data are sparse, it is not always clear how best to choose cut-off points; the grouping we have shown results in a finding that is more compatible with the findings for duration and for intensity of exposure. It would be interesting to see if a regrouping of the exposure categories alters the results of the analyses based on internal comparisons.

References


Burns replies

We appreciate the interest taken in our study by Freedman. At the heart of the discussion are the interpretation of the significance of the statistics in our study,\textsuperscript{1} the lack of significance in others. A critical point in valuing causation is the weight of the evidence to be placed upon the non-significant increase of non-specific exposures found in human studies of amyotrophic lateral sclerosis, compared with the weight placed upon controlled animal studies specific to the herbicide 2,4-dichlorophenoxyacetic acid (2,4-D).

In three with Freedman the undue reliance upon significance is ill advised. He is correct that the case-control studies cited in our paper showed increased odds ratios,\textsuperscript{5} but there is no evidence that any subjects were actually exposed to 2,4-D. The exposures were limited to pesticides, agricultural chemicals, and herbicides. The cohort studies examined workers who were definitely exposed to 2,4-D and thus provide a more valid assessment of risk even though they are less powerful than the case-control studies.\textsuperscript{3} The cohort studies of 2,4-D do not consistently show increased risk of ALS.

The associations found in the case-control studies are clearly unsupported by the experimental studies that have been conducted on 2,4-D. Environmental causes of ALS remain unknown. If future epidemiological studies investigate the neurotoxicity of herbicides such as 2,4-D, the researchers must improve upon the status quo of surrogate exposure information used in case-control studies or perform further studies of the 2,4-D workers. Epidemiologists must make a commitment to quality exposure assessment of individual pesticides, perhaps coupled with biomonitoring, to assess the putative health concerns associated with pesticides.

CJ BURNS
The Dow Chemical Company, 1803 Building, Midland, MI 48674, USA

Correspondence to: C J Burns cburns@dow.com
Bullying in hospitals

As victims of bullying and proponents of emotional intelligence in the health profession we read with interest the article on workplace bullying.1 Kivimaki et al1 did not mention whether the responses were anonymous. Identified responses may underestimate the incidence of bullying in the cohort. Given that previous studies (mentioned by the authors in the discussion) have shown a considerable percentage of victims deciding to resign as a result of bullying, it is a pity that the article by Kivimaki et al did not contain similar data. The other two issues that should have been included were the duration of the bullying, and how many bullies are actually aware that they are bullies. These can be answered by asking the question: Have you subjected your colleagues to such bullying behaviour?

With doctors and nurses constituting 58% of the victims, we wonder whether the authors could reanalyse their data to see whether there is a higher incidence of bullying in the high stress specialties—such as adult intensive care and neonatal intensive care. We would also like to know whether the victims in their study were offered any counselling by their institutions, and if so, the nature and impact of the counselling.

Emotional intelligence is defined by the five emotional quotients of self awareness of feelings, emotional self regulation, self monitoring and goal setting, empathy, social skills, and communication skills.1 According to Goleman, “The rules for work are changing, we’re being judged by a new yardstick: not just how smart we are, or our expertise, but also how well we handle ourselves and each other.”3 Emotional intelligence is considered more important than intelligence quotient (IQ) in enabling people to function well in society.5 We suggest that emotional intelligence, which can be taught, can be an important solution in reducing the incidence of bullying in the workplace.6

T S KOH
T H H G KOH
EQ-InSight and Kirwan Hospital, QLD 4814, Australia

Correspondence to: Dr T H H G Koh
guan_koh@health.qld.gov.au
